

SHIPS THAT PASS IN THE NIGHT

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A major source of tension between Staddon's *The new behaviorism* and Baum's *Review* is that the former was written for a general audience but the latter evaluates it as a technical work. Be that as it may, the central issue—Skinner's conception of the role of theory in behavior analysis—is inadequately portrayed in both the book and the review. The two primary sources of difficulty arise from failures to honor Skinner's distinction between experimental analysis and interpretation and to appreciate Skinner's views on events that are not observable at the behavioral scale of measurement.

Key words: conceptual nervous system, experimental analysis, interpretation, reinforcement theory, theoretical behaviorism

In my view, *The New Behaviorism* (TNB) is correct that behavior analysis must expand its theoretical horizon, but is incorrect that “theoretical behaviorism” meets this challenge. Baum's review is correct that TNB mischaracterizes Skinner's views of the place of theory in behavior analysis, but is incorrect that the type of theoretical terms favored by TNB and the review is consistent with behavior analysis. Both TNB and the review err in their shared criticisms of Skinner's exploration of the societal implications of the experimental analysis of behavior and his treatment of the effects of punishment.

Before developing these points, I note the source of many of the differences between TNB and the review: TNB is self-described as an effort to “appeal to the general scientific reader” (p. xiii), but the review judges TNB almost exclusively as a technical work. In seeking to appeal to a general audience, TNB sometimes incompletely represents Skinner's views and includes tangential (but interesting) topics; for example, the purported relation between the philosophy of behaviorism and the “postmodern aesthetic” in literature. As a book intended for a general audience, TNB largely succeeds in form if less so in substance. The broad scholarship of the author is everywhere evident and his ideas are engagingly presented. As examples: “debate dissolves error, silence crystallizes it” (p. 33) (which I trust applies to the present exchanges as well); “when it comes to scientific re-

search, plausibility or a catchy name often trumps validity” (p. 56); “if operant conditioning is a sculptor, she is a sculptor in wood, with its grain and knots, rather than a sculptor in isotropic clay” (p. 71); “always it is the organism that ‘proposes’ and the environment that ‘disposes’ ” (p. 116); “the ability to simulate mentalistic theories with quantitative precision conferred a Good Housekeeping Seal of Approval on cognitive psychology” (p. 126) (damning mentalism with faint praise); “most Hullians and Tolmanians embraced the cognitive revolution when it roared down the pike, concepts blazing” (p. 147); and “if theories are fallible, so are ‘facts’ ” (p. 150). Those who know John Staddon or have heard him present papers at meetings will recognize his voice in this book, with all its charm and acerbic wit. TNB is a good read, if not always a reliable guide to behaviorism as formulated by Skinner.

The Place of Theory in Behavior Analysis

It is true that a given statement of Skinner considered in isolation might mislead the reader about the proper place of theory in behavior analysis. However, to believe that Skinner could be so obviously wrong-headed about such an important matter is to believe that Skinner was either careless or stupid. Neither of these possibilities seems as likely as the alternative—that we have misunderstood him. What are the causes of this misunderstanding? In my view, the major source of confusion is the conflict within Skinner between his intention to establish an independent science of behavior that could be used to explain the world outside the laboratory (a largely pragmatic/political endeavor) and

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his intention to develop an experimentally based, natural science of behavior (a largely principled/scientific endeavor). Skinner's dual pragmatic and principled concerns sometimes led different statements to be directed toward different audiences. When Skinner neglected to specify explicitly the primary audience for a statement, its meaning became ambiguous. Was the statement intended as a heuristic to guide day-to-day practice or as a fundamental principle to inform the scientific enterprise? When Skinner intended the former, but was misinterpreted to intend the latter, the door was opened for critics to claim that Skinner had asserted every absurdity that he had not specifically denied. Skinner assumed that we were as careful and intelligent as he was—and equally familiar with his entire body of work—but these assumptions were sometimes too generous.

To illustrate the difficulty that can ensue when pragmatic heuristics are mistaken for fundamental principles, consider one concept—reinforcement. Reinforcers are conventionally defined as stimuli that increase the subsequent frequency of the responses with which they were contingent. This definition is driven primarily by pragmatic considerations: An applied behavior analyst or an experimental analyst need only employ one of a number of previously identified stimuli, such as social approval in the first instance or food for a deprived animal in the second, to increase the strength of behavior that precedes the stimulus. However, the pragmatic definition of reinforcement is often taken for a principled statement—that it is the *only* statement about reinforcement that is consistent with behavior analysis. This view is reflected in the claim in *TNB* that “the Skinnerian system is almost silent on what makes a reinforcer reinforcing” (p. 50). However, there is nothing in the “Skinnerian system” that requires the pragmatic definition to be taken as a principled statement. In fact, there are obvious reasons why it should not. First, the definition is insufficient—even on pragmatic grounds—because a given stimulus may function as a reinforcer for one organism at one moment but not for a second organism, and not for the same organism at another moment. Second, the definition does not allow a given stimulus to be identified as a putative reinforcer prior to the establishment of

the response-reinforcer contingency (cf. Meehl, 1950). Recognizing the insufficiency of a purely pragmatic definition, behavior analysts have developed several conceptually related principles of reinforcement, including differential response-probability theory (Premack, 1959), response-deprivation theory (Timberlake & Allison, 1974), and unified reinforcement theory (Donahoe, Burgos, & Palmer, 1993; Donahoe, Crowley, Millard, & Stickney, 1982; cf. Rescorla & Wagner, 1972). All of these theories are stated in terms of the moment-to-moment relations among events favored by Skinner; none appeals to “events taking place somewhere else, at some other level of observation, described in different terms and measured, if at all, in different dimensions” (Skinner, 1950, p. 193).

Although behavior analysis has not been “silent” about reinforcement theory, the misidentification of a pragmatic definition of reinforcement for a principled statement has retarded progress in our field. With only a few exceptions (e.g., Williams, 1975), the experimental analysis of behavior was slow to appreciate the fundamental significance for reinforcement theory of the blocking effect (Kamin, 1968, 1969). This neglect occurred despite the fact that experimental analysts had uncovered some of the earliest evidence of blocking (Johnson & Cumming, 1968; Thomas, 1970) and had directly confirmed its occurrence in the operant paradigm (vom Saal & Jenkins, 1970). Applied behavior analysis, on occasion, has also suffered from the failure to incorporate blocking, as in the case of fading procedures. The procedures instituted in fading are somewhat similar to those used in blocking, although the first facilitates stimulus control whereas the second hinders it: Both fading and blocking involve acquisition of a discrimination followed by transfer to subsequent discriminations whose stimuli are similar to those of earlier ones. A justly well-regarded applied behavior analyst attempted to train a pigeon to turn around when a panel was illuminated with the word TURN and to peck when the panel was illuminated with PECK. (I shall follow Skinner's “Golden Rule” and not credit a source if one would not wish it to be credited to oneself.) In an attempt to facilitate acquisition of this difficult visual form discrimination, the two words were initially displayed in different col-

ors (red and black) with the red portions of one word progressively replaced with black. All proceeded well, even when only a very small part of one of the letters was red. However, when the last bit of red disappeared, stimulus control disappeared along with it. Blocking, not fading, had inadvertently been implemented: Control by the shape of the letters had been blocked by their color. A theoretical analysis of reinforcement that was informed by blocking might have averted this outcome.

Theoretical Behaviorism and Behavior Analysis

TNB argues that “a viable behaviorism must go beyond observables . . .” (p. 151). What is the evidence for this claim, and what is the nature of the unobservables?

Are hidden variables necessary? Both *TNB* and Staddon’s reply offer two sorts of evidence for unobservables—everyday examples and experimental findings. Neither is sufficient in my view. As the everyday example, “If ‘behavior’ is ‘uninterpreted physical movement,’ then we cannot distinguish between ‘waving’ and ‘drowning’ or any of the myriad other cases where the same physical event has different significance at different times” (Staddon, 2004, p. 82). (The single quotation marks denote phrases from *TNB* that are repeated in Staddon’s reply.) This statement commits the same error as occurs in linguistics when it is claimed that “bat” in the phrases “the bat hit the ball” and “the bat bit the boy” are the same word. Both *TNB* and linguistics incorrectly view responses as providing a proper unit of analysis. However, responses are not selected by reinforcers, environment–behavior relations are selected (Donahoe et al., 1993; Donahoe & Palmer, 1994). As Skinner noted, “It is the nature of [operant] behavior that . . . discriminative stimuli are practically inevitable” (Skinner, 1937, p. 273; see also Catania & Keller, 1981, p. 163 and Dinsmoor, 1995, p. 52). Waving when seeing a friend and gesturing while drowning may have the same response topography (although this is debatable), but they are not members of the same operant class because they are under the control of very different stimuli.

The experimental result said to require “unobservables” is no more persuasive, and

for the same reason. The finding is that an eliciting stimulus presented at brief interstimulus intervals (ISIs) produces greater habituation than a stimulus presented at longer ISIs but that, after a delay, habituation is greater to the stimulus previously presented at the longer ISI. The disjunction between the effects of the rate of stimulus presentation on habituation and on retention of habituation is said to “require some kind of memory” such that “after the short-ISI series, most of the memory strength is in the fast-decaying memory, whereas after the long-ISI series, most is in the slow-decaying memory” (*TNB*, pp. 156, 157). These different memories are conceptualized as different “internal” or “hidden states.” The problematic core of this account can be found in its failure to recognize that the two habituation procedures produce different environment–behavior relations: During habituation, the elicited response occurs in different stimulus contexts when it is evoked at short ISIs than at long ISIs. The context during the short-ISI procedure includes the recent presentation of the preceding eliciting stimulus. During the test procedure, which follows a delay period, the eliciting stimulus occurs in a stimulus context that is less similar to the habituation context for the short-ISI stimulus than for the long-ISI stimulus. Thus habituation to the short-ISI stimulus is reduced because the test context does not reinstate the context in which the eliciting stimulus appeared during original habituation training. In short, the differential effects of short and long ISIs on the retention of habituation can be interpreted by a behavioral analysis that does not appeal to internal states but, instead, exploits the well-documented effects of stimulus generalization on responding. Indeed, such an account of the effects of the ISI on habituation has been offered (Donahoe & Wessells, 1980, pp. 54–60).

Although the evidence upon which *TNB* argues for the necessity of hidden states is not persuasive, a compelling case can be made on other grounds (Donahoe, 2001). It is in this respect that theoretical behaviorism is onto something. (For a more complete presentation of *TNB*’s argument, see Staddon, 2001b). There are indeed phenomena, such as positive and negative patterning, that are not amenable to theoretical treatments whose

terms are restricted to environmental and behavioral events alone. In positive patterning, a response is reinforced in the presence (or absence) of two stimuli (e.g., a light and a tone) but not in the presence of either stimulus alone. In negative patterning, a response is reinforced in the presence of either stimulus alone, but not in their joint presence (or absence). The mere occurrence of a patterning discrimination does not necessarily pose a problem for a strictly behavioral analysis: One could regard the compound of two stimuli as forming a distinct stimulus and thereby conceptualize a patterning procedure as a multiple schedule that consists of various combinations of stimulus components and their compounds. What is problematic is that organisms acquire positive patterning much more readily than negative patterning, even when the net stimulus intensity of the components and the compound is equated (e.g., Bellingham, Gillette-Bellingham, & Kehoe, 1985). If the compound and its components are simply distinct stimuli, why should generalization from the compound to the components be different than from the components to the compound? Formal analysis has shown that complex discriminations such as patterning require hidden variables whose values are affected—not by the environment directly—but by other variables whose values are directly or indirectly affected by the environment (e.g., Minsky & Papert, 1969). (See Donahoe, 2001, for a more extensive discussion of this and related issues.) Although hidden variables are not required for all discriminations, one cannot claim that hidden variables are engaged only when certain complex discriminations are encountered and not otherwise. Organisms do not have a priori knowledge of the nature of a discrimination upon its first occasion and, therefore, cannot engage hidden variables only as needed (cf. Donahoe et al., 1993, pp. 21–22). Moreover, every stimulus appears within some context and, therefore, stimulus compounds and hidden variables are ubiquitous.

What is the nature of hidden variables? Given that hidden variables are necessary for the theoretical analysis of some environment–behavior relations, what is the nature of such variables? How should they be conceptualized? The position taken in Staddon’s reply is

that hidden variables are “internal states, . . . which means . . . ‘internal to the model,’ not, or at least not necessarily, ‘internal to the organism’ ” (Staddon, 2004, p. 81). That is, hidden variables in theoretical behaviorism are conceptualized as inferences from behavioral observations. In this respect, the hidden variables of *TNB* are epistemologically indistinguishable from the inferred constructs of cognitive psychology. They are instrumental fictions that make no existential claim; in other words, they are variables whose meaning is exhausted by the functional relations into which they enter with other variables (cf. MacCorquodale & Meehl, 1948). Surprisingly, the review appears to welcome such constructs into the behavior-analytic fold: “That sounds all right; state variables are useful in models” (Baum, 2004, p. 75). Thus the review does not object to hidden variables per se, but to *TNB*’s view of their nature (i.e., their ontological status).

In *TNB*, Staddon goes back and forth on the question of the ontological status of hidden variables. Three possibilities are considered—intervening variables (abstract terms in formal models), behavioral variables, or physical variables (biological events that are internal to the organism). I consider each, in turn, from the perspective of behavior analysis as formulated by Skinner. Intervening variables were anathema to Skinner because they were not products of independent experimental analyses but of inferences from the very behavior they sought to explain (Skinner, 1950; but see the concept of “reflex reserve,” Skinner, 1938, p. 26). Behavioral observations, by themselves, insufficiently constrain intervening variables because a given environment–behavior relation can be produced by any of a large number of underlying processes. Logically, the insufficiency of behavioral inferences as the basis for intervening variables is clear; otherwise one would be indifferent between studying physiology with the methods of behavior analysis or neuroscience. The underdetermination of the many-to-one mapping of physiology to behavior invites circular reasoning and the nominal fallacy (Donahoe & Palmer, 1994, pp. 9, 152; Skinner, 1938).

Formal analysis has confirmed Skinner’s intuition about the inability to infer internal states from knowledge of only the inputs and

outputs of a system. As one example, consider the analysis of Markov decision processes (MDPs), much simpler systems than the nonlinear neuromuscular systems of living organisms. In MDPs, some of the intervening states in a multistep process may not be directly observable; that is, such MDPs have hidden states that do not correspond to observable input (I) or output (O) states. Formal research has shown that the I-O relations of a MDP with hidden states can be simulated by another MDP whose hidden states do not correspond to the hidden states of the MDP being simulated. Thus the "true" hidden states cannot be validly inferred from a correspondence between the I-O relations of the simulation and of the system being simulated (e.g., Jaeger, 1998; Kaelbling, Littman, & Cassandra, 1998; see also Moore, 1956, cited in *TNB*). In the words of a major contributor to the MDP literature, "I would think it is fundamentally hopeless to try to deduce the 'right' internal machinery from I-O observations" (H. Jaeger, personal communication, May 9, 2001). Conceptualizing hidden states as intervening variables is inconsistent with both Skinner's views and formal analysis, even though *TNB* and the review both appear to endorse them.

Instead of regarding hidden states as intervening variables, can they be considered behavioral states as *TNB* suggests? As noted in the review, *TNB* sometimes claims that "these models then *are* the behavior, . . . what the organism is 'doing' . . ." (*TNB*, p. 144), a claim that the review rightly rejects. *TNB*'s hidden states are not treated in the way that Skinner treated hidden states (i.e., "private events"). For Skinner, private events entered into only the types of functional relations that had been uncovered through prior experimental analysis of *public* events (Skinner, 1957, p. 11). That is, private events are controlled by their antecedents and maintained by their consequences. As an example, a subvocal tact might be controlled by the sight of an object and reinforced by stimuli produced by responding (either overtly or covertly) in a manner that had previously been reinforced by the verbal community in the presence of that object, including by conditioned reinforcers arising from the listener's own behavior. In contrast, *TNB*'s hidden states are assigned characteristics that are not

directly tied to physiology and do not have the dimensions of behavior—leaky integrators with charge times, decay constants, and the like.

Finally, can hidden variables be conceptualized as states in the real nervous system? In Staddon's reply, he does not rule out the possibility that such states *might* reflect biological states that are, in fact, internal to the organism. Paradoxically, the review accepts hidden variables but—contrary to Skinner—only if they are intervening variables. The surprising endorsement of such theoretical terms is defended on grounds that physiological variables run afoul of Skinner's dictum against "appeals to events taking place . . . at some other level of observation . . ." (Skinner, 1950, p. 193).

In the full context of Skinner's writings, I believe that both *TNB* and the review have mistaken Skinner's pragmatic reservations about "events taking place . . . at some other level of observation" for a principled rejection of the potential contributions of such events *if they are the product of experimental observations*. In Staddon's reply, he remarks, "If all Skinner meant was to argue against naive neurophysiology . . . he should have said so" (p. 81). I believe that Skinner did say so. Immediately following Skinner's initial comments on the relation between physiology (he called it "neurology") and behavior, he illustrated his general point with an example from within physiology—the relation between inhibition and the reflex arc (Skinner, 1938, p. 418 ff; cf. Donahoe & Palmer, 1988). Skinner did not object to attributing changes in reflex strength (events at one level) to variations in inhibition (events at a lower level) but to appealing to such events in the absence of their direct measurement. "The correlation of a physicochemical process, *once it is observed* [italics added] with inhibition at the level of . . . [reflexes] . . . will require a rigorous quantitative formulation at these latter levels . . . I am not overlooking the advance that is made in the unification of knowledge when terms at one level of analysis are defined ('explained') at a lower level. Eventually, a synthesis of the laws of behavior and of the nervous system may be achieved . . ." (Skinner, 1938, pp. 423, 428). Skinner emphasized the independence of a science of behavior from neuroscience, but

he did not deny the potential relevance of neuroscience at the margins of behavioral phenomena (such as patterning discriminations, for example) or the potential benefits of such a synthesis.

Moreover, if hidden variables were to be considered, they had to be the product of direct observation at the level of the variable. Skinner's core objection was to a Conceptual Nervous System whose elements were *inferences* from higher-level observations and were not themselves directly measured. Skinner explicitly and consistently regarded himself as a biological scientist from his time as a graduate student with the biologist William Crozier through his later years. "The experimental analysis of behavior is a rigorous, extensive, and rapidly advancing branch of biology . . ." (1974, p. 255). "The physiologist of the future will tell us *all that can be known* [italics added] about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily 'historical'—that is to say, it is confined to functional relations showing temporal gaps. . . . It will make the picture of human action more nearly complete. What [the physiologist] discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete" (1974, pp. 236–237). In short, Skinner asserted (a) the independence of a science of behavior from the science of physiology, (b) the interrelation of the two sciences, paralleling the relation between other sister sciences such as physiology and biochemistry, and (c) the requirement that variables hidden from behavioral observation must be products of direct observations at the physiological level. To achieve "the advance that is made in the unification of knowledge when terms at one level of analysis are defined ('explained') at a lower level," hidden variables had to arise from independent experimental analysis. Behavior analysis can tolerate unobserved variables, but not unobservable ones.

Societal Implications of Behavior Analysis

My concern here is not with whether Skinner's conjectures about the societal implications of the experimental analysis of behavior were correct (although I believe that many were), but with the kind of enterprise they

represent. *TNB* claims that the goal of all behavior analysis is "prediction and control" (p. 23) but that extrapolations to human behavior are severely limited because "human behavior cannot be predicted with anything like the precision required" (p. 86). These statements are represented as Skinner's views, but they are fundamental distortions of them. Skinner distinguished between two interrelated aspects of behavior analysis, or of any science for that matter—experimental analysis and interpretation (Donahoe & Palmer, 1989; Palmer & Donahoe, 1992). Prediction and control are the provinces of experimental analysis, not of interpretation. Toward the end of *The Behavior of Organisms*, Skinner commented: "The reader will have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs. But it is a serious, though common, mistake to allow questions of ultimate application to influence the development of a systematic science at an early stage. . . . The book represents nothing more than an experimental analysis of a representative sample of behavior. Let him extrapolate who will" (1938, pp. 441–442). For Skinner, experimental analysis entails the ability to manipulate or control all of the variables that affect a given behavior and to measure all of the effects of the manipulation that enter into orderly functional relations (cf. Skinner, 1950, 1966). He was clear that only under such idealized circumstances were prediction and control strictly possible. Scientific interpretation was a different matter. As stated in *Verbal Behavior*, Skinner's most extensive interpretation, "The emphasis is upon an orderly arrangement of well-known facts, in accordance with a formulation of behavior derived from an experimental analysis of a more rigorous sort. The present extension to verbal behavior is thus an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results" (1957, p. 11). Further clarification is provided in *About Behaviorism* (1974) where he noted, "Much of the argument goes beyond the established facts. I am concerned with interpretation rather than prediction and control" (p. 21).

"Our knowledge . . . is limited by accessibility, not by the nature of the facts. . . . As in other sciences, we often lack the information necessary for prediction and control and must be satisfied with interpretation, but our interpretations will have the support of the prediction and control which have been possible under other conditions" (p. 194). "We cannot predict or control human behavior in daily life with the precision obtained in the laboratory, but we can nevertheless use results from the laboratory to interpret behavior elsewhere" (p. 251). Thus, contrary to *TNB*, Skinner did not "take prediction and control to be the be-all and end-all of psychology" (2001a, p. 148), but of experimental analysis only.

During Skinner's time, interpretation was largely restricted to verbal interpretations that used ordinary language to trace the implications of experimental-analytic principles. The circumstances were much like those that prevailed in evolutionary biology before the advent of population genetics. Population genetics permitted the implications of Mendelian genetics for evolution to be more fully explored (cf. Donahoe, 2003). Recently, quantitative methods such as neural networks (Donahoe & Palmer, 1988) have emerged that promise to reveal the implications of behavior-analytic principles in a more precise and persuasive form than ordinary language can achieve. Although *TNB* does not necessarily endorse the incorporation of neuroscience when devising such methods, their potential is recognized: "Not a few traditional cognitive psychologists are properly uneasy about these new developments, which they correctly perceive as a resurrection of behaviorism under a new guise" (2001a, p. 137). Behavior analysts may also be reluctant to embrace these methods because of the historical conjunction of superficially similar quantitative procedures and cognitive psychology. Ironically, Thomas Hunt Morgan—the "father of the gene"—was once similarly reluctant to incorporate genetics into the study of heredity:

In the modern interpretation of Mendelism, facts are being transformed into factors [i.e., genes] at a rapid rate. If one factor will not explain the facts, then two are invoked; if two prove insufficient, three will sometimes work out. The superior jugglery sometimes necessary to account for the results may blind

us . . . to the commonplace that the results are so excellently 'explained' because the explanation was invented to explain them. We work backwards from the facts to the factors, and then, presto! Explain the facts by the very factors that we invented to account for them. (Morgan, 1909; cited in Shine & Wrobel, 1976, p. 51)¹

It was only after Morgan's *direct observation* of the giant chromosomes within cells of the salivary gland of the fruit fly that he welcomed the "hidden variable" of the gene into the experimental analysis of heredity.

Punishment and Behavior Analysis

I cannot quit this commentary without a few remarks on the jointly expressed concerns of *TNB* and the review about Skinner's views of punishment. Frankly, I fail to see what all the fuss is about. Skinner concluded on the basis of the experimental evidence available to him that (a) some stimuli (termed punishers) decreased the probability of operants with which they were contingent, (b) other stimuli with which punishers had been contingent could function as conditioned stimuli for escape and avoidance if they became contingent with operant behavior, and (c) the operant with which the punisher had been contingent would recover in strength after extinction of the escape/avoidance behavior conditioned to stimuli that also controlled the operant (Estes & Skinner, 1941; Skinner, 1953, 1974). Although the review claims that "Skinner . . . was wrong about the facts on punishment . . ." (p. 75), I detect no error in Skinner's empirical conclusions. Later work did demonstrate that more intense punishers could effectively reduce the strength of operants for very long periods of time (e.g., Church, 1969; Solomon & Wynne, 1954), but this finding amplifies rather than contradicts Skinner's conclusions. It is true that these new facts have sometimes been taken as evidence that punishers reduce responding in ways that strictly parallel those by which reinforcers increase responding (the so-called symmetrical law of effect) (Rachlin & Herrnstein, 1969). However, this is an *interpretation* of the facts and is not required by them. For example, unified reinforcement

¹I am indebted to my colleague John J. B. Ayres for bringing this reference to my attention.

theory can accommodate both reinforcement and punishment with the same principle, but the behavioral processes differ by which these effects arise (Donahoe, 2003, Donahoe & Palmer, 1994, pp. 114–115).

TNB's reservations about Skinner's views of punishment appear to stem less from the findings than from Skinner's interpretations of the findings. "Punishment is termed by Skinner *aversive control* and the one clear value that emerges from his writings is unequivocal opposition to it" (p. 108). *TNB* then proceeds to question whether punishment is always less effective than reinforcement and whether punishment uniquely has unwanted side effects relative to reinforcement, neither of which Skinner claimed so far as I am aware. (Is this another case in which Skinner is assumed to have asserted what he did not specifically deny?) Instead of asserting that punishment was ineffective, Skinner stressed that punishment provides an extremely effective reinforcer for the behavior of the person administering the punishment (i.e., a punisher terminates aversive stimuli occasioned by the offender's behavior). Note that Skinner's emphasis implicitly entails the proposition that punishers are, indeed, effective—at least in the short run: The stimuli produced by the offender's behavior would not cease unless the punisher did, in fact, eliminate the behavior. Skinner also emphasized the possible unwanted "side effects" of punishment—the conditioning of responses evoked by the punishing stimulus and subsequent escape or avoidance from the person instituting punishment. It is true that he did not discuss the possible untoward side effects of reinforcement, but the finding that stimuli which usually function as reinforcers may sometimes elicit responses that compete with the operant (as when food contingent with a dry mouth fails to increase the frequency of maintaining a dry mouth; Sheffield, 1965) is not inconsistent with Skinner's admonitions about the unintended side effects of punishment (cf. Donahoe & Palmer, 1994, p. 52). Staddon concludes his discussion of punishment with an acknowledgement of the limitations of interpretation, limitations with which Skinner would undoubtedly concur: "I am not sure whether these . . . speculations on the societal effects of punishment are true or not. Arguments like this are based on a

combination of intuition and some laboratory experiments. They can never be conclusive" (p. 119).

I began this commentary with the title of a poem by Paul Lawrence Dunbar (1872–1906), "Ships That Pass in the Night." The title was intended to suggest that *TNB* and the review often talk past one another, addressing largely different issues on largely different grounds. It is perhaps appropriate that the commentary conclude with the words of another 19th century poet, Francis Thompson (1859–1907). The citation is intended to suggest that Skinner remains our surest beacon for the future, and that his influence is inescapable. The title of the poem is "The Hound of Heaven," and, although Skinner was no god, he is as near to one as our science is likely to have.

"I fled him, down the nights and down the days;
I fled him, down the arches of the years; . . .

"Lo, all things fly thee, for thou fliest me!
Strange, piteous, futile thing! . . .

"Ah, fondest, blindest, weakest,
I am he whom thou seekest!"

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